

Comment on “One Loop Renormalization of Soliton Quantum Mass Corrections in 1+1 Dimensional Scalar Field Theory Models” (Phys. Lett. B542 (2002) 282 [hep-th/0206047])

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We refute the claim that previous works on the one-loop quantum mass of solitons had incorrectly dropped a surface term from a partial integration. Rather, the paper quoted in the title contains a fallacious derivation with two compensating errors. We also remark that the $\phi^2 \cos^2 \ln(\phi^2)$ model considered in that paper does not have solitons at the quantum level because at two-loop order the degeneracy of the vacua is lifted. This may be remedied, however, by a supersymmetric extension.

The issue of quantum corrections to solitons, both in bosonic and in supersymmetric theories, has in the last few years received extensive examination and a number of subtleties have been revealed and clarified. In a recent publication in Physics Letters B, Ref. [1], a formula for the quantum mass of (1+1)-dimensional solitons at one-loop order has been put forward, which includes a surface term from a partial integration that allegedly had not been considered in other (and therefore incorrect) treatments. In this Comment we wish to make it clear (a) that the previous works quoted in Ref. [1] did include this surface term where appropriate, (b) that this surface term arises only in certain regularization prescriptions for sums of zero-point energies, and (c) that the actual derivation presented in Ref. [1] is fallacious and leads to the correct result only because of two compensating errors, which unfortunately can also be found in the widely used textbook [2], as two of us have pointed out already in a footnote in Ref. [3].

Whereas in the case of solitons in scalar (1+1)-dimensional field theories the correct one-loop results are known from the classic papers of Ref. [4,5], the

situation is much more involved and subtle in the corresponding supersymmetric (SUSY) models. In fact, contradictory results [6–16] have dominated the early literature on one-loop corrections to (1+1)-dimensional SUSY solitons, which in part were due to a surprising sensitivity to the regularization method even when the renormalization conditions have been fully fixed [3]. This finding has triggered a number of investigations, and the subtleties of the various methods that can be employed to calculate the one-loop corrections of (1+1)-dimensional SUSY solitons have been sorted out in every detail only rather recently [17–27]. It is therefore unfortunate that a new paper on the simpler (1+1)-dimensional scalar models with solitons, where some of these issues already arise, ignores these subtleties and misinterprets the literature that has in fact resolved them. We therefore deem it necessary to put things right and thus prevent the possible spread of new confusion about the status of the various methods to calculate one-loop corrections to soliton masses.

In the notation of Ref. [1], the one-loop contribution to the quantum mass of a soliton arises from the difference in zero-point energies in the presence of the soliton and in the vacuum,

$$\Delta M_{\text{bare}} = \frac{1}{2} \sum_i \omega_i + \frac{1}{2} \sum_q \omega(q) - \frac{1}{2} \sum_k \omega^0(k), \quad (1)$$

where i and q labels the eigenfrequencies of the discrete and continuum modes in the presence of a soliton, and k those of the vacuum modes. The energy-momentum relation is formally the same for the soliton and the vacuum sector, $\omega(q) = \sqrt{q^2 + m^2}$ and $\omega^0(k) = \sqrt{k^2 + m^2}$, but in the soliton background there is an additional phase shift $\delta(q)$ which gives rise to different densities of states.

Because the sums over zero-point energies are quadratically divergent, they, as well as their difference, which still contains a logarithmic divergence, require careful regularization. In mode regularization [4] the system is put into a finite box, and if one considers an equal (and finite) number of modes in the two sectors, one can, as done in Ref. [1], consider individual differences of zero-point energies with given (e.g. periodic) boundary conditions, leading to

$$\sqrt{q_n^2 + m^2} = \sqrt{k_n^2 + m^2} - \frac{k_n \delta(k_n)}{L \sqrt{k_n^2 + m^2}} + O(L^{-2}). \quad (2)$$

Inserting this in (1) and using the free density of states $L/(2\pi)$, Ref. [1] states that an integration by part gives

$$\Delta M_{\text{bare}} = \frac{1}{2} \sum_i \omega_i - \frac{\omega_k \delta(k)}{4\pi} \Big|_{-\infty}^{\infty} + \frac{1}{2} \int_{-\infty}^{\infty} \frac{dk}{2\pi} \omega(k) \frac{d}{dk} \delta(k), \quad (3)$$

and that the resulting surface term was not considered in other treatments, for example [28], [21], [3].

While this latter part of the statement is correct (if empty, see below) as concerns Ref. [28], it is incorrect for the other two references given at that point. In particular, Ref. [3] discusses this surface term at length and moreover shows that the above derivation, which can also be found in the textbook of Rajaraman [2], is not correct. The above derivation treats $\delta(k)$ as a continuous function, but then it cannot vanish for $k \rightarrow \pm\infty$ because a continuous phase shift function has $\delta(+\infty) - \delta(-\infty) = -2\pi\mathcal{N}$, where $\mathcal{N} \geq 1$ is the number of discrete modes, and the surface term would be divergent. In this case one has to compare k_n and q_n with shifted mode numbers such that at large k the effective phase shift in (2) tends to zero [3]. An alternative is clearly to adopt a discontinuous $\delta(k)$ such that $\delta(k \rightarrow \pm\infty) \rightarrow 0$, and one may choose to put the discontinuity at $k = 0$ (see Fig. 1). In this approach there is a further surface term contributing to (3), namely

$$\left. \frac{\omega_k \delta(k)}{4\pi} \right|_{0-}^{0+} = \frac{1}{2} m \mathcal{N}.$$

With a finite number of modes in both sectors, and the requirement of their equality, this extra contribution is cancelled because the vacuum sector has \mathcal{N} more continuous modes than the soliton sector, and these need to be subtracted explicitly, as discussed in detail in Ref. [17,21]. Clearly, these considerations require regularization (a finite mode number cutoff which is taken to be the same in the two sectors), but Ref. [1] skips the stage where the regulator is still in place.

Now it is true that in other treatments the above surface terms are not considered. The reason is simply that they arise only in mode regularization in a finite volume. If one refrains from introducing a finite volume to make the spectrum discrete, one naturally *starts* from considering the difference of the spectral densities which is just given by $\delta'(k)$ and uses this to integrate over

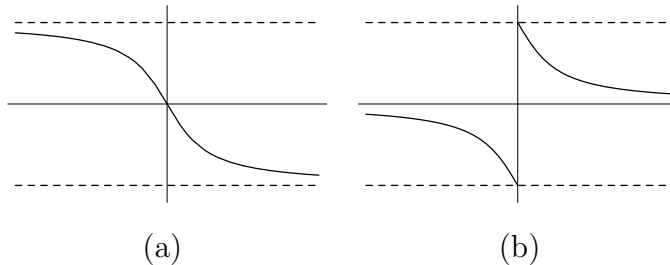


Fig. 1. Schematically, two possible choices for the phase shift function $\delta(k)$: (a) continuous, with $\delta(+\infty) - \delta(-\infty) = -2\pi\mathcal{N}$, $\mathcal{N} \geq 1$; (b) discontinuous with $\delta(0+) - \delta(0-) = 2\pi\mathcal{N}$. Ref. [1] implicitly assumed both, a continuous behaviour and $\delta(k \rightarrow \pm\infty) \rightarrow 0$, which cannot be had at the same time.

the continuous spectrum, i.e. (3) *without* the surface term. This only makes sense if the k -integration is regularized, and explicit calculation for example in dimensional regularization [29,26] does give the correct result.

Simple energy/momentum cutoff regularization in such a continuum formulation, on the other hand, is more dangerous, as has been shown in Ref. [3], and it has led to incorrect results in the early literature on SUSY solitons. It can, however, be repaired by introducing a sharp cutoff on the phase shift function as a limit of smooth ones [20], which produces in fact an additional term equivalent to the one in (3). However, it is not correct that this additional term is the same as the surface term encountered in mode number regularization, as suggested by the author of Ref. [1] who implies that the error of a naive momentum cutoff was simply the omission of this surface term. As we have seen, for a phase shift function with $\delta(k \rightarrow \pm\infty) \rightarrow 0$, the surface terms that Ref. [1] should have included involve an additional contribution at low momentum that is not generated by smoothing out the high momentum cutoff. And in momentum cutoff regularization these extra terms are not cancelled as in mode number regularization, because the former by definition does not require equality of the number of modes but instead equality of the momentum cutoff in the soliton and vacuum sector. The problem of naive momentum cutoff regularization is therefore exclusively that of a sharp UV cutoff. As an aside, we have recently demonstrated [26] that a smooth UV cutoff on the phase shift function is in fact required to obtain finite results in higher-dimensional kink domain walls; only in 1+1 dimensions a sharp UV cutoff presents a real pitfall in that it leads to finite (but incorrect) results.

We would like to emphasize once more the importance of regularization in the problem of quantum corrections to soliton masses. In order to understand and resolve the subtleties arising in this case, it is mandatory to avoid manipulation of unregulated and thus ill-defined quantities. This is particularly necessary in the supersymmetric case: In Refs. [3,17] two of us have encountered the problem that nonzero corrections to the quantum mass of SUSY solitons are seemingly not matched by those in the central charge operator as required by BPS saturation [30]. In Ref. [17] we conjectured the existence of an anomaly, and this was subsequently established by Shifman et al. [19] in the form of an anomalous additional term in the central charge operator. On the other hand, in Ref. [18] Graham and Jaffe have put forward a proof that the central charge and the mass of SUSY solitons receive the same corrections, apparently without the need for an anomalous contribution. However, Ref. [18] arrived at this conclusion by formal manipulation of unregularized quantities, invoking (dimensional) regularization only at a later stage. In Ref. [27] we have recently resolved this apparent contradiction and demonstrated how the anomalous contribution to the central charge of the SUSY kink arises from a careful implementation of dimensional regularization.

Finally, we would like to remark that Ref. [1] also contains an interesting generalization to non-reflectionless potentials and the associated field theory models in 1+1 dimensions. As one particular example, a $V = \phi^2 \cos^2 \ln(\phi^2)$ model is considered which has been introduced in Ref. [31]. Such a model has infinitely many degenerate vacua, accumulating about $\phi = 0$ which is a non-analytic point. At one-loop order, the degeneracy of these vacua at $\phi \neq 0$ is not lifted because all minima have equal curvature V'' , and only this enters in the one-loop effective potential. However, at two-loop order the quantum corrections are proportional to $(V''')^2$, which is larger for the minima with smaller value of ϕ (and diverges for $\phi \rightarrow 0$). As a consequence, at the quantum level neighboring minima are no longer degenerate and the corresponding solitons do no longer exist. It does therefore, unfortunately, make no sense to calculate quantum corrections to the solitons as these only exist in the (semi-)classical theory. However, a possibility for keeping solitons at the quantum level in such a model might be to consider its supersymmetric extension as in Ref. [32], where a similar problem has been encountered in a simpler model and at one-loop level. We intend to study this question further in a separate work.

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